

MERCK INSTITUTE
FOR
THERAPEUTIC RESEARCH
RAHWAY, N. J.

April 6, 1954

Dr. Joshua Lederberg
University of Wisconsin
College of Agriculture
Department of Genetics
Madison 6, Wisconsin

Dear Doctor Lederberg:

I sent off the resistant *Gratia* and was unfortunately interrupted and had to go to Chicago before I had a chance to write to you about it. Upon my return I found your letter of the 27th. The resistant culture we are now dealing with is not the same one reported on by Smith (J. Bact. 58, 761), the latter having been lost. Actually this is about the third separate isolate of a resistant from this strain. Its $Q_0(N)$ s are the following: Endogenous 8, pyruvate 54, pyruvate + oxalacetate 96, and malate 42. This represents the resistant pattern and I naturally assumed that this strain would show essentially the growth pattern of the earlier. Your data shows that this is not the case. I also set up a similar experiment in a yeast-extract tryptone medium with three degrees of aeration - i.e. 10 ml. in the usual tube, 10 ml. in a 250 ml. flask, not shaken, and the same, shaken. The growth at 18 hours was:

Without Glucose (Growth in μ g. bact. N/ml.)

	<u>Sensitive</u>	<u>Resistant</u>
Tube	36	36
Stationary	174	71
Shake	560	485

With 1% Glucose

Tube	150	42
Stationary	310	148
Shake	690	680

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-2-

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While there is a slight difference between the resistant and sensitive strains at the low and intermediate degrees of oxidation, there is essentially no difference under conditions of high aeration which presumably were the conditions used by Smith et al. It seems apparent that we do not now have a strain showing these properties. The question I suppose is whether we ever had one, for if we claim they exist we ought to be able to demonstrate the phenomenon. I feel sure that we had some once, but am not inclined to go hunt for one again inasmuch as the point has nothing to do with the mechanism of resistance nor with the action of streptomycin, which is the point of interest to me.

Sincerely yours,


Wayne W. Umbreit

WWU:VMF